ISSN 1988-088X

Departamento de Fundamentos del Análisis Económico II

Department of Foundations of Economic Analysis II
University of the Basque Country
Avda. Lehendakari Aguirre 83
48015 Bilbao (SPAIN)
http://www.dfaeii.ehu.es

DFAE-II WP Series

2010-04

Pablo Brañas, María Paz Espinosa

Unravelling Public Good Games:
The Role of Priors

Unraveling Public Good Games: The role of priors*

Pablo Brañas–Garza
Universidad de Granada

María Paz Espinosa
Universidad del País Vasco

February 19, 2010

Abstract

This paper provides experimental evidence on how players predict end game effects in a linear public good game. Our regression analysis yields a measure of the relative importance of priors and signals on subjects' beliefs on contributions and allow us to conclude that, firstly, the weight of the signal is relatively unimportant, while priors have a large weight and, secondly, priors are the same for all periods. Hence, subjects do not expect end game effects and there is very little updating of beliefs.

Keywords: public good game, end game effect, beliefs
JEL Class.: C91, D64, C72, H41.

*We gratefully acknowledge comments and suggestions from Jordi Brandts, Werner Guth, Vitoria Levatti, Maite Martínez-Granado and participants at the Max Planck Institute Jena seminar and Alhambra Experimental Workshop. María Paz Espinosa acknowledges financial aid from MCI (SEJ2006-06309/ECON) and Gobierno Vasco, DEUI (IT-313-07); Pablo Brañas-Garza from MCI (SEJ2007-62081/ECON).

1 Motivation

Previous experimental research on public good games has shown that in one-shot games contributions are relatively high (40%-60% of endowment) and they fall over time in finitely repeated public good games (see Davis and Holt, 1993; Isaac, McCue and Plott, 1985; Kim and Walker, 1984; Ledyard, 1995). Deviations from the free-riding zero contribution outcome and the decline over time have been rationalized through *social preferences*, *learning effects*, *strategic considerations* or *conditional cooperation.*[1] Binmore (2006) offers an explanation based on social norms. In the case of inexperienced laboratory subjects, the framing of the game triggers a social norm coming from an indefinitely repeated game. The experience acquired in the game through trial and error adjustments changes behavior and may explain the decline in contributions.

Cooperation may survive in an infinitely repeated game, but even in a finite game, if there is a small probability that some subjects are not fully rational, rational subjects may react by contributing in the early periods and stop contributing toward the end of the game (see Kreps et al., 1982). Players may not want to trigger a break in cooperation when the others are contributing, but, of course, this argument is no longer valid as the end of the game approaches and, in particular, when lowering the contribution in the last period cannot trigger any retaliation. This argument can be extended. If players were aware that there would be no consequence from lowering contributions in the last period, and they thought others were likewise aware, they might also realize that lowering contributions in the previous to last period would not trigger any retaliation either. This unraveling would make the finite game equivalent to a one-shot game but it requires common knowledge of rationality. The question is therefore whether subjects do indeed solve the game by backwards induction. There is some evidence that subjects find it difficult to apply this type of reasoning. Palacios and Volij (2008) find that agents used to the backwards induction arguments (chess players) applied it when playing the centipede game, while subjects who were more unfamiliar with it (students) did not use it to the same extent. Using

[1] See Andreoni (1988, 1995), Houser and Kurzban (2002), Chaudhuri and Paichayontvijit (2006), Ma, Sherstyuk, Dowling and Hill (2002), Keser and Winden (2000), Brandts and Schram (2001), Offerman, Sonnemans and Schram (1996) and Janssen and Ahn (2006), among others.

backwards induction seems to require some learning. The usual laboratory experiment repetitions of the PGG will not provide that learning since subjects face the end of the game only once. Johnson, Camerer, Sen and Rymon (2002) have shown that subjects taught to use backward induction made equilibrium offers in an alternative offer bargaining game when playing with robots; however, when they played with untrained subjects they behaved differently, although closer to the equilibrium offers than prior to training. They conclude that both social preferences and a limited use of backward induction play a role in the discrepancy between the experimental outcome and the equilibrium prediction.

Problems with backwards induction are not the only cognitive difficulties faced by players. Understanding the incentives in the one-shot game may also be an issue. Most papers have focused on this last type of limited cognition and on how learning through repetitions of the one-shot PGG mitigate its effects (see Anderson et al, 1998; Andreoni, 1988, 1995; Brandts and Schram, 2001; Goeree et al, 2002; Houser and Kurzban, 2002; Palfrey and Prisbey, 1996, 1997). However, little attention has been paid to another source of cognitive limitation in PGG: the fact that subjects are not used to applying backward induction arguments in finite games, nor do they believe that other subjects will use this type of reasoning. To analyze this problem, we focus on the end-game effect[2] in PGG and subjects' beliefs in this effect.[3] Our work confirms the difficulties related to backwards induction arguments in finite PGG.

Our main result indicates that a majority of subjects do not predict any end game effect at all, even when beliefs are elicited after playing the game. We model ex-post beliefs as a linear combination of prior beliefs and the signals observed during the game. In this set up, we find that the signal has a low weight in determining ex-post beliefs and, even though subjects experienced an end-game effect, this effect is absent from their ex-post beliefs.

The rest of the paper is organized as follows. Section 2 describes the experimental design and procedures. Section 3 presents our main results on average behavior and beliefs. In Section 4, we analyze individual behavior.

[2]Several papers have dealt with the question of *end-game effects*. Gonzalez et al. (2005) find that replacing a definite endpoint with an interval, whether commonly or privately known, does not change the timing of defection nor the average contribution levels.

[3]Several papers explore beliefs –and elicitation mechanisms– in PGG (see for instance Gätcher and Renner 2006, Dufwenberg et al. 2006, Iturbe-Ormaetxe et al. 2007, Kovarik 2008).

Section 5 concludes.

2 Experimental design

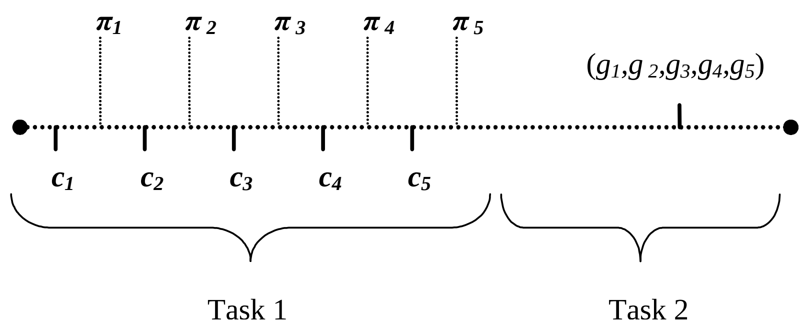
The experiment was carried out in a single session at Universidad de Granada on May 31^{st}, 2007. Participants were first year undergraduate students in Economics. The total number of participants was 48 divided in 12 groups. Students were told that they would perform several tasks. (See the Instructions in the appendix).

For the first task, subjects played a linear public good game (PGG) in each group for five periods. Subjects were informed that they would be playing with the same partners for the five periods. In each period, the subjects were given an endowment of 100 2-eurocent coins. They were asked to make a decision on how much to allocate to a private account and how much to allocate to a public account. Contributions were expressed in number of coins, thus, they were integer numbers between 0 and 100, $c_{it} \in [0, 100]$. Participants were informed that they could keep any money allocated to the private account for themselves, and this would be independently of the other subjects' actions, while all the money allocated to the public account (the sum of the money allocated by the four members of the group) would be multiplied by 1.5 and then divided equally among the four members. Each participant earned the sum of payoffs obtained in the five periods.

After each period, each subject received feedback privately on his own payoff, π_{it}. Before the new period started they were given a new endowment of 100 2-eurocent coins. Figure 1 summarizes the timing of the experiment.

After making decisions on contributions to the public account for 5 periods, and getting feedback on their payoffs, subjects started Task 2.

Figure 1: Timing



In Task 2, they were asked about their beliefs regarding the average contribution to the public account (in number of coins) of the 48 participants and for each of the five periods (g_{it}). We used an incentive scheme according to their errors, $\varepsilon_{it} = intc_t - g_{it}$ (being $intc_t$ the integer of the observed average and $t = 1, ..., 5$). More precisely,[4]

- If $|\varepsilon_{it}| > 10$ participant i did not receive anything;
- If $5 < |\varepsilon_{it}| \leq 10$ participant i received 1 euro;
- If $0 < |\varepsilon_{it}| \leq 5$ participant i received 2 euros;
- Finally, if $\varepsilon_{it} = 0$ participant i would receive 20 euros.

Participants were told that only one of the periods chosen at random would determine their payoff for Task 2.

We did not perform a belief elicitation step before Task 1 to avoid any possible effects on contributions.[5] Since our main interest was to determine how priors are affected by the experience of playing the game and whether the priors or the posterior beliefs incorporate any end-game effect, we chose

[4]Alternative reward functions include quadratic and linear scoring rules and other procedures that correct for risk attitudes (Karni, 2009). The interval schedule was used for the simplicity of explaining the rule. It is similar to scoring rules that provide a positive reward for an exact prediction and zero otherwise (Charness and Dufwenberg (2006) and Dufwenberg, Gächter and Hennig-Schmidt (2006), among others, have used these rules). These scoring rules elicit the mode of the subjective probability distribution for a risk neutral subject (see Andersen, Fountain, Harrison and Rutström (2009)). Since we are eliciting forecasts on the *average* contribution of subjects, it is likely that subjective distributions are unimodal and symmetric.

[5]The evidence on whether belief elicitation may affect contribution is mixed. See for example Gächter and Renner (2006).

a design with a low number of periods. This design makes the end-effect very important in the game experience and increases the chances of observing it in the forecasts. Also, subjects had enough time to think what they would do; after each decision a few minutes were left, then the feedback about payoffs was received and then the following period would start. In Task 2 subjects had the feedback received in the five periods at their disposal, so that any possible differences in recall between subjects could not introduce any noise in the results. Note that subjects do not observe contributions but may infer the level of group contributions very easily multiplying the feedback by 4 and dividing over 1.5. Even if they did not explicitly calculate contributions, note that our purpose is not to determine whether subjects are accurate in their predictions, only if they could predict the end game effect. If they observed a decline in profits from the public account, this could only come from a decline in contributions. We did not provide data on contributions to avoid the implicit suggestion that they should use the average of the group in their predictions. This design allows us to measure the relative importance of priors, on the one hand, and the subjects' experience in Task 1 (group signals), on the other, for the subjects' forecasts in Task 2. The complete experiment lasted about an hour and subjects earned, on average, €13.47.

3 Average Behavior

We first compared actions and elicited beliefs. We checked whether subjects, who had played the PGG for five periods and had received feedback about their own payoff after each period, could accurately predict the mean contribution of the population and to what extent they could predict any end-game effects. Since forecasts were elicited in Task 2, they will be called posterior beliefs.

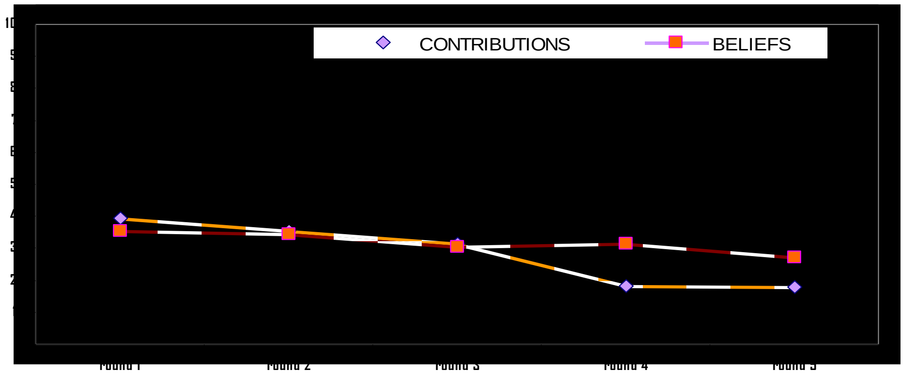
Figure 2 shows both the average posterior beliefs (over the whole population) and average actions in each period in the 4–player public good game.[6] The average of contributions in the first three periods is 35.3, not very different from the average forecasts, 33.4. In the last two periods, however, there

[6] The observed values were:

- contributions: 39.3, 35.4, 31.4, 18.4 and 17.9.
- beliefs: 35.3, 34.4, 30.6, 31.4 and 27.3.

is a discrepancy between average contributions (18.1) and beliefs (29.3) suggesting that the end game effect observed in contributions in the last two periods was not predicted in Task 2.

Figure 2: CONTRIBUTIONS (TASK1) AND BELIEFS (TASK2)



We checked whether there is a significant change from one round to the next. Table 1 explores the evolution of actions and beliefs. We used the Wilcoxon test to check differences between c_t and c_{t-1} (g_t and g_{t-1}).

There was a significant decline in contributions between periods 3 and 4 (see also Figure 2) but this trend did not continue to period five. We did not see a similar declining pattern for beliefs.

Table 1: Evolution of c_t and g_t. ($n = 48$)

	Z	$p-value$		Z	$p-value$
$\mathbf{c}_1, \mathbf{c}_2$	-0.02	0.98	$\mathbf{g}_1, \mathbf{g}_2$	-0.25	0.80
$\mathbf{c}_2, \mathbf{c}_3$	-0.19	0.84	$\mathbf{g}_2, \mathbf{g}_3$	-1.30	0.19
$\mathbf{c}_3, \mathbf{c}_4$	-2.34*	0.01	$\mathbf{g}_3, \mathbf{g}_4$	-0.15	0.88
$\mathbf{c}_4, \mathbf{c}_5$	-0.52	0.60	$\mathbf{g}_4, \mathbf{g}_5$	-1.69	0.09

Observe that whereas subjects changed their behavior in period 4, this change was not incorporated into posterior beliefs and subjects overstated the value of the participants' contribution at the end of the game.

To explore differences between actions and beliefs in period t we define the error or discrepancy between them as e_{it}, $e_{it} = c_t - g_{it}$, $(t = 1, 2, ..., 5)$. Table 2 summarizes these discrepancies.

Table 2: BELIEFS ACCURACY

	mean	*median*	*st. dev.*
$\mathbf{e}_1$	4.02	4.01	19.98
$\mathbf{e}_2$	0.97	5.39	18.29
$\mathbf{e}_3$	0.81	4.39	17.89
$\mathbf{e}_4$	-13.04	-10.63	18.17
$\mathbf{e}_5$	-9.37	-5.63	17.01

Recall that positive values indicate low guesses. The mean difference between actions and beliefs was relatively small for the first three periods. However, the average difference increased in periods 4 and 5 and became negative. Subjects did not predict end game effects, and contributions and guesses diverged.

Hence, subjects' beliefs matched actions fairly well for the first three rounds but failed to do so in T and $T-1$. In period 4, when the end game effect was first observed, the difference between the two is statistically significant.[7]

We may conclude that, concerning average behavior:

Result 1a) There was an end-game effect at period $T-1$.

1b) On average, players did not incorporate end-game effects to their posterior beliefs.

Result 1 refers to average behavior. However, different types of players may follow different patterns.[8] We will address this issue in the next section.

4 Individual results

Figure 2 shows the extent of the end-game phenomenon in aggregate beliefs and contributions. Now we analyze individual behavior and beliefs to

[7] We checked whether c_t and g_t are drawn from the same population using paired non–parametric test. The Wilcoxon test compares c_t and g_t for each round. Z_1=–0.50 ($p-value$ = 0.61); Z_2=-0.11 (0.91); Z_3 =-0.28 (0.77); Z_4=-3.83 (0.00) and Z_5=-2,96 (0.00). Sign tests yield similar results.

[8] Previous work on PGG has shown evidence of subjects' heterogeneity. For instance, Fischbacher *et al.* (2001) and Chaudhuri and Paichayontvijit (2006) found that some players are conditional cooperators and others are free-riders.

explore the question more deeply. Figure 3 shows the histogram for the discrepancies between contributions and guesses for each period. Non-negative errors are represented on the right of the graphs. Individuals on the left area overestimated mean contributions, that is, they were optimistic.

The percentage of subjects over and underestimating contributions is balanced for periods 1 to 3. However, after round 4 the percentage of subjects with optimistic predictions increased notably and broke the balance.

Figure 2 shows that there was an end-game effect at period 4, but the mean predictions did not incorporate it. We may conclude from Figure 3 that not only the mean but a relevant percentage of the individuals did not predict this decline.

To improve our understanding of these phenomena we will now focus on the period when they lowered contributions and the period when they believed the end game phenomenon would occur. We define a decrease in contributions as lowering the contribution to a value (*i*) lower than $\frac{2}{3}$ of the previous value and (*ii*) lower than $\frac{2}{3}$ of the average of the own contribution in previous periods, provided the decrease is maintained up to the last period.[9] Identical criterion is used for guesses.

- contributions: 25% (12 out of 48) of subjects decreased their contribution in period 4, 12.5% defected at period 5, but a high percentage of subjects (23%) did not decrease their contribution as the end of the game approached.

- beliefs: 25 out of 48 subjects (52%) did not predict any end game effect; 10 subjects (21%) believed that the end game effect would occur at the last period and only one made the right prediction (decline at period 4).

[9] The actual decrease in the average contribution in period 4 was from (39.3; 35, 4; and 31.4) to (18.4; 17.9) which fulfills this criterion. Small changes in the threshold do not change results (choosing 0.6 or 0.7 leaves results almost unchanged).

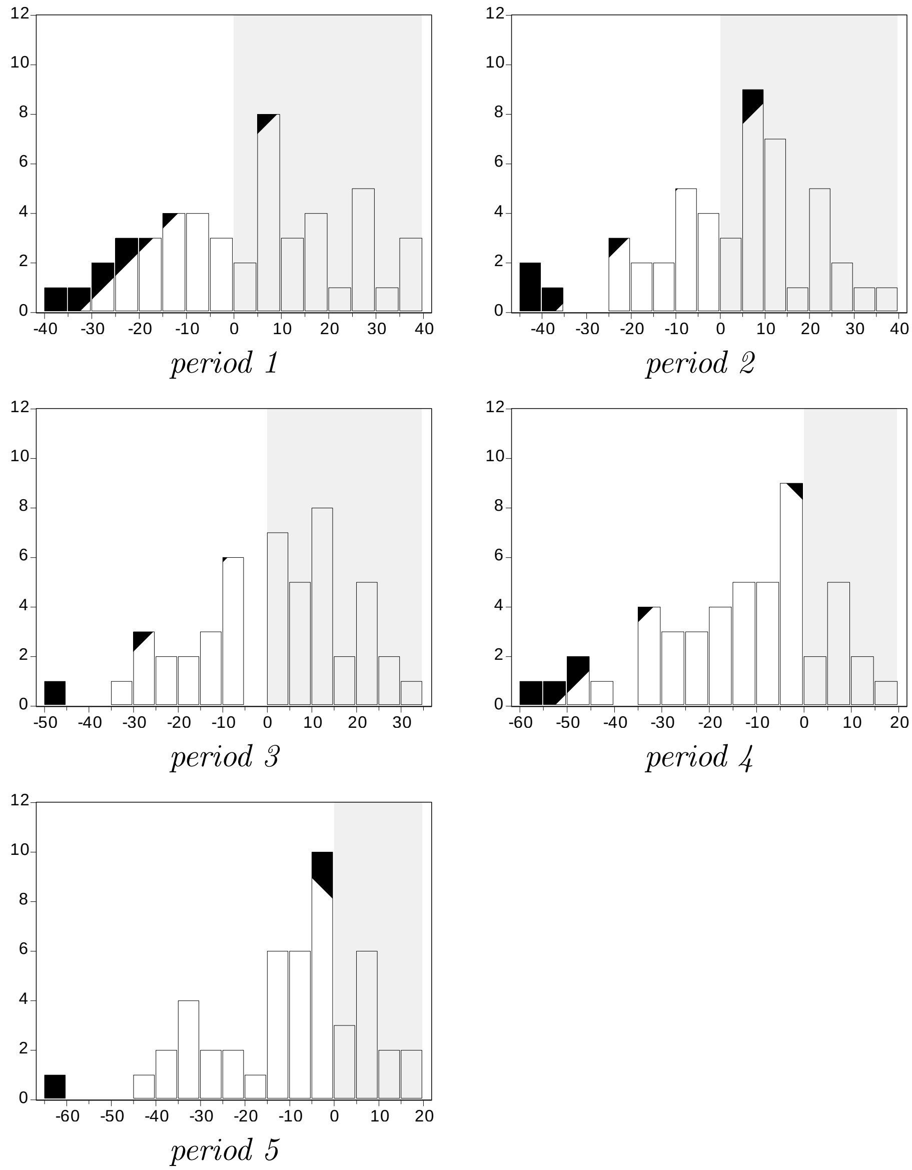


Figure 3: HISTOGRAMS FOR $c_t - g_{it}$

This means that 73% (35 out 48) of the players either predicted the decrease in contributions later than the period in which the decrease took place or they did not predict it at all. This is remarkable since at the time of the prediction they had already seen the outcome of the five periods of the contribution game in their own group of four subjects (although the prediction referred to the average of all participants). Subjects had the opportunity to update their beliefs with the observed behavior in their group, in case they had not predicted ex ante the end-game effect.

Result 2) Half of the subjects did not incorporate the observed end game effects into their posterior beliefs.

We will now try to rationalize this result by looking at how posterior beliefs are formed. Beliefs were elicited after playing the PGG so that they must be a combination of ex-ante beliefs (priors) and the signals observed throughout the game. Subjects did not observe other players contributions, but they did observe the part of the payoff that comes from their group contributions to the public account. We define this value as the signal[10] observed by individual i in group z at each period t:

$$\mathbf{s}_{it} = \frac{1.5 \sum_{i \in z} c_{it}}{4}$$

We model ex-post beliefs as a linear combination of prior beliefs and the signal observed in the game for each individual i; the weights represent the relative importance of each source of information:

$$\mathbf{g}_{it} = (1 - \alpha) p_{it} + \alpha \mathbf{s}_{it},$$

where prior beliefs of individual i, p_{it}, might vary across periods.[11]

As we observe $\mathbf{s}_{it}$ and $\mathbf{g}_{it}$ we can obtain an estimation of α. The assumption is that the weight given to the signal and that given to the priors are the same for all individuals. We estimate the following panel regression with fixed effects:

[10] An alternative signal could be the subjects' payoffs (private + public account). We also used this variable as the signal (see footnote 14).

[11] This expression for the updating of beliefs is justified as follows: If the individual has n information sources on the value of the average contribution c: $(x_1, ...x_n)$ such that $x_i \sim N(c, \sigma_i^2)$ the quality of the ith information source is given by its precision $\psi_i = \frac{1}{\sigma_i^2}$. Then, the conditional expectation of c satisfies: $E[c/x_1, ...x_n] = \sum_{i=1}^{n} \psi_i x_i \big/ \sum_{i=1}^{n} \psi_i$, that is, the weighted average of the different information sources. Thus, a rational player would weight the priors and the signal according to the relative quality of each information source $\alpha_i = \frac{\psi_i}{\psi_1 + \psi_2}$.

$$\mathbf{g}_{it} = \underbrace{\left[\gamma_o + \gamma_i + \sum_{t=2}^{5} \beta_t d_t\right]}_{(1-\alpha)p_{it}} + \alpha \mathbf{s}_{it} + u_{it} \qquad [1]$$

where γ_o is the constant, γ_i are individual fixed effects (reflecting subjects' heterogeneity), and d_t are period dummies allowing priors to be different across periods; the estimated parameters $\widehat{\beta}_t$ will be an indication of how individuals predict the end game effect (if they do, parameters $\widehat{\beta}_4$ and $\widehat{\beta}_5$ will be negative and significant), and u_{it} is the error term.

Table 4 shows the regression results.

Table 4: Regression Results. Beliefs $\mathbf{g}_{it}$

beliefs (g_{it})		(1)		(2)
signal (s_{it})	0.11*	(0.04)	0.14*	(0.03)
constant	28.80*	(3.11)	25.97*	(1.68)
d_2	−0.21	(2.38)		
d_3	−3.38	(2.42)		
d_4	−0.41	(2.74)		
d_5	−4.49	(2.76)		
	$R^2 = 0.09$	$n = 240$	$R^2 = 0.07$	$n = 240$
	$F = 4.15$	$p - val. = 0.00$	$F = 14.98$	$p - val. = 0.00$

(*) significant at 1%; (std. errors).

The period dummies are not significant. The implication is that when we separate the effect of the priors and that of the signal, priors are not time dependent, i.e., on average subjects did not predict ex ante a decline in contributions over time.

Eliminating the time dummies from the regression yields the coefficients shown on the right of Table 4, regression (2). The estimated weight of the signal, $\widehat{\alpha}$, is 14% and the weight of the priors, $1 - \widehat{\alpha}$, is six times larger.[12]

In regression (2) we may obtain a measure of each individual prior beliefs weighted by $(1 - \alpha)$ through the predicted constant and fixed effects: $\widehat{\gamma}_o +$

[12] If we normalize signals to reflect average contribution of the group: $\frac{\sum_{i \in z} c_{it}}{4}$, the estimated coefficient of the normalized signal is 0.21.

$\widehat{\gamma}_i$ (see equation [1]). We calculate the main statistics for the (predicted) prior beliefs: the mean (std. dev.) is 25.97 (14.47) and the max (min) is 69.3 (-0.8).[13] Therefore, we observe a large heterogeneity in priors across subjects.

Summing up our results in this section,

Result 3a) Priors are constant across periods, that is, subjects did not predict ex ante any end game effects. There is a large heterogeneity in priors.

3b) In the formation of posterior beliefs, the weight given to the signal ($\widehat{\alpha}$) is relatively low: $10\% - 15\%$. Priors are given a much larger weight.

To check the robustness of this result, we considered two alternative signals that the subject could use to update his priors: the own contributions or the payoff he received in each period. However, these signals turned out not to be significant.[14] In sum, subjects do not use their own contribution or the payoff as signals to form their posterior beliefs, but the average contribution of their group.

The low weight given to the signal is consistent with the fact that although individuals experienced an end game effect, they did not guess it after the game. Other papers have found evidence in the same direction: subjects barely update their beliefs (see Kovarik, 2008). Given the low weight given to signals, we should not expect large learning effects from repetitions of a finite PGG.[15]

5 Discussion

In the experimental literature on PGG, repetition of the one-shot game has been shown to decrease contributions. Repetition introduces learning effects,

[13] The unweighted values are 30.20 and 80.58, respectively.

[14] Using the individual payoffs as signals yields a coefficient 0.02 ($p - value$ 0.61); for individual contributions the coefficient is 0.04 ($p - value$ 0.25). Adding subjects' contributions or payoffs in regressions (1) and (2) does not change results substantively in terms of the estimated coefficient of s_{it}.

[15] This is also consistent with the low speed of learning observed in the centipede game (see Palacios and Volij, 2008).

strategic considerations and the possibility of punishment for the unfair behavior of others[16] that could be related to the decrease in contributions.

We contribute to this literature on experimental public good games with the idea that the subjects' abilities to unravel the game, and their beliefs on the ability of others to do so, may be an important factor behind the experimental results. We performed this analysis by asking subjects about their beliefs regarding average contributions for each period. The belief elicitation was conducted after the PGG to avoid any interference with contributions.

Our regression analysis allowed us to measure the relative importance of priors and signals on subjects' belief formation. Our main results are that priors are constant for all periods and they have a significant weight compared to the signals observed throughout the game.

Our analysis suggests that, prior to playing the game, subjects do not expect backward induction, not even in the last few periods, and their updating using the observed signals is slow. Therefore, the posteriors beliefs do not incorporate the end-game effect.

Previous papers have studied the reasons behind contributions: kindness, altruism or warm-glow vs. errors or confusion (see Croson, 2007; Andreoni 1995; Houser and Kurzban, 2002, among others). Our paper focuses on a different kind of confusion: people are not able to predict end game effects. However, this confusion is not inconsistent with individuals endowed with *other-regarding* preferences and, more precisely, with subjects who consider that other players could have social preferences.

References

[1] Andersen, Steffen, Fountain, John, Harrison, Glenn W. and E. Elisabet Rutström (2009). Estimating Subjective Probabilities. Working Paper 09-01, Department of Economics, College of Business Administration, University of Central Florida.

[2] Anderson, Simon P., Goeree, Jacob and Charles A. Holt (1998). A Theoretical Analysis of Altruism and Decision Error in Public Goods Games. *Journal of Public Economics* **70**(2): 297-323.

[16] See Andreoni (1995) and Houser and Kurzban (2002).

[3] Andreoni, James (1995). Cooperation in public-goods experiments: kindness or confusion?. *The American Economic Review* **85**(4): 891-904.

[4] Andreoni, James (1988). Why Free Ride? Strategies and Learning in Public Goods Experiments. *Journal of Public Economics* **37**(3): 291-304.

[5] Binmore, Ken (1997). Rationality and backward induction. *Journal of Economic Methodology* **4**: 23-41.

[6] Binmore, Ken (2006) Why Do People Cooperate. *Politics, Philosophy and Economics* **5**(1): 81–96.

[7] Bosch-Domenech, Antoni, García Montalvo, Jose, Nagel Rosemarie and Albert Satorra (2002). One, two, (three), infinity, . . . : Newspaper and lab beauty-contest experiments. *American Economic Review* **92**(5):1687–1701.

[8] Brandts, Jordi and Arthur Schram (2001). Cooperation or Noise in Public Goods Experiments: Applying the Contribution Function Approach. *Journal of Public Economics* **79**: 399-427.

[9] Charness, Gary and Matthew Rabin (2002). Understanding social preferences with simple tests. *Quarterly Journal of Economics* **117**: 817-869.

[10] Charness, Gary and Martin Dufwenberg (2006). Promises and Partnership. *Econometrica*, **74**(6): 1579-1601.

[11] Chaudhuri, Ananish and Tirnud Paichayontvijit (2006). Conditional cooperation and voluntary contributions to a public good. *Economics Bulletin* **3**(8): 1-14.

[12] Croson, Rachel (2007). Theories of Commitment, Altruism and Reciprocity: Evidence from Linear Public Goods Games. *Economic Inquiry* **45**:199-216.

[13] Davis, Douglas and Charles A. Holt (1993). Chapter 6. Experimental Economics, Princeton: *Princeton University Press.*

[14] Dufwenberg, Martin, Gächter, Simon and H. Hennig-Schmidt (2006). The Framing of Games and the Psychology of Strategic Choice, *CeDEx Discussion Paper* No. 2006-20.

[15] Fischbacher, Urs, Gachter, Simon and Ernst Fehr (2001). Are people conditionally cooperative? Evidence from a public goods experiment, *Economics Letters* **71**(3): 397-404.

[16] Gächter, Simon and E. Renner (2006). The effects of (incentivized) belief elicitation in public goods experiments, *CeDEx Discussion Paper* No. 2006–16.

[17] Goeree, Jacob, Holt, Charles A. and Susan Laury (2002). Private Costs and Public Benefits: Unraveling the Effects of Altruism and Noisy Behavior. *Journal of Public Economics* **83**(2): 257-278.

[18] González, Luis G., Güth, Werner and M. Vittoria Levati (2005). When does the game end? Public goods experiments with non-definite and non-commonly known time horizons. *Economics Letters* **88**(2):221-226.

[19] Houser, Dan and Robert Kurzban (2002). Revisiting kindness and confusion in public goods experiments, *The American Economic Review* **92**(4): 1062-1069.

[20] Iturbe-Ormaetxe, Íñigo, Ponti, Giovanni, Tomás, Josefa and Luis Ubeda (2007). Is Prevention Better than Cure? Framing Effects in Public Good Provision, *University of Alicante*, Mimeo.

[21] Isaac, Mark R.,Walker, James M. and Arlington W. Williams (1994). Group size and the voluntary provision of public goods. *Journal of Public Economics* **54**: 1-36.

[22] Isaac, Mark R., McCue, Kenneth F. and Charles R. Plott (1985). Public goods provision in an experimental environment, *Journal of Public Economics* **26**: 51-74.

[23] Janssen, Marco A. and T. K. Ahn (2006). Learning, Signalling and Social Preferences in Public-Good Games, *Ecology and Society* 11(2):art.21.

[24] Johnson, Erik, Camerer, Colin, Sen, Sankar and Talia Rymon (2002). Detecting failures of backward induction, *Journal of Economic Theory* **104**: 6-47.

[25] Karni, Edi (2009). A Mechanism for Eliciting Probabilities. *Econometrica* **77**(2): 603-606.

[26] Keser, Claudia and Frans van Winden (2000). Conditional Cooperation and Voluntary Contributions to Public Goods, *Scandinavian Journal of Economics*,

[27] Kim, Oliver and Mark Walker (1984) The free rider problem: Experimental Evidence, *Public Choice* **43**: 3-24.

[28] Kovarik, Jaromir (2008). Belief Formation and Evolution in Public Good Games, *Universidad de Alicante*, Mimeo.

[29] Kreps, David, Milgrom, Paul, Roberts John and Robert Wilson (1982). Rational Cooperation in the finitely repeated prisoners' dilemma, *Journal of Economic Theory* **27**: 245-252.

[30] Ledyard, John O. (1995). Public Good: a Survey of Experimental Research. In: The Handbook of Experimental Economics (eds. J. H. Kagel and A. E. Roth), Princeton: *Princeton University Press*.

[31] Ma, Leanne, Sherstyuk, Katerina, Dowling, Malcolm and Oliver Hill (2002) Altruism and voluntary provision of public goods, *Economics Bulletin*, Vol. 3, No. 31: 1-8.

[32] Meier, Stephan (2007). A Survey of Economic Theories and Field Evidence on Pro-Social Behavior. In: Economics and Psychology: A Promising New Field (eds. B. S. Frey and A. Stutzer), Cambridge: MIT Press.

[33] Merlo, Antonio and Andrew Schotter (1992). Experimentation and Learning in Laboratory Experiments: Harrison's Criticism Revisited, *American Economic Review* **82**(5): 1413-1425.

[34] Nagel, Rosemarie (1998). A survey on experimental "beauty-contest games": Bounded rationality and learning. In: D. Budescu, I. Erev, and R. Zwick, editors, *Games and Human Behavior, Essays in Honor of Amnon Rapoport* 105–142. Lawrence Erlbaum Associates, New Jersey.

[35] Offerman, Theo, Sonnemans, Joep and Arthur Shram (1996). Value Orientations, Expectations and Voluntary Contributions in Public Goods, *The Economic Journal* **106**: 817-845.

[36] Palfrey, Thomas R. and Jeffrey E. Prisbey (1996). Altruism, Reputation, and Noise in Linear Public Goods Experiments. *Journal of Public Economics* **61**: 409-27.

[37] Palfrey, Thomas R. and Jeffrey E. Prisbey (1997). Anomalous Behavior in Linear Public Goods Experiments: How Much and Why?. *American Economic Review* **87**: 829-46.

[38] Palacios-Huerta, Ignacio and Oscar Volig (2008). Field Centipedes. *American Economic Review*, forthcoming.

[39] Rabin, Matthew (1993). Incorporating Fairness into Game Theory and Economics, *American Economic Review* **83**: 1281-1302.

[40] Sonnemans, Joep, Schram, Arthur and Theo Offerman (1999). Strategic Behavior in public good games: when partners drift apart, *Economics Letters* 62, 1, p. 35-41.

[41] Stahl, Dale and Paul W. Wilson (1995). "On Players' Models of Other Players: Theory and Experimental Evidence". *Games and Economic Behavior* **10**: 218-254.

[42] Wilcox, Nat (2006). Theories of learning in games and heterogeneity bias. *Econometrica* **74**:1271-1292.